

November 13, 1950.

Dr. Pierre Fredericq,
Institute of Bacteriology,
University of Liege,
Belgium.

Dear Dr. Fredericq:

Thank you very much for sending the set of reprints, which arrived several days ago. I have been studying them diligently, and profitably, since receiving them.

My previous impression, that there was an unlimited series of distinct colicins, seems to be incorrect in view of your classification of 17 main types. In view of this, I wonder if it would not be simpler for you to send a representative culture of each colicin type, and your basic sensitive strain, such as CA 81.

I well appreciate that my request for transshipping 18 cultures might put you to some considerable trouble and that you might well wish some justification for such a request. My primary interest in colicins is to use them as genetic testers, since the mutations for resistance to different colicins appear to be essentially independent. Thus, 17 colicins would, in principle add 17 additional genetic markers for use in our recombination studies. I am particularly interested in application to a new development, namely crosses between distinct strains of *E. coli*, which have recently been shown to be feasible. The genetic differences existing between strains as isolated (as opposed to their experimental induction in the laboratory) is a matter of some interest to the geneticist as well as the taxonomist, and differences in colicin responses promise to have some use in this direction.

On page 84 of your "These d'agregation", I note a reference to colicin-sensitivity of *S. typhimurium*. One of my students has been studying recombination in this species, with results which barely constitute definitive evidence for recombination between a few strains. His major difficulty is a paucity of simple genetic markers. Have you encountered specific antibiotic actions against this species, aside from the colicins already recognized?

Your expressed interest in the genetic aspects of variation in respect to colicins leads me to make the following, perhaps presumptive suggestion, which I hope you will forgive. A number of the problems discussed in Part 3 of your review would seem to be especially amenable to recombination study, particularly the question of "mutations associees". The methodology of recombination is not difficult, and can readily be applied to such questions as the genetic identity (i.e. allelism) of different mutations, or whether two "associated" changes are based on a single genetic change. As a partial compensation for the assistance you have promised, I would be delighted to send you suitable, intercrossable cultures of *E. coli* K-12, and the very simple procedure for their use.

Sincerely,

Joshua Lederberg